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ABSTRACT

Using "Patterns of Childrearing," by Sears, Maccoby and Levin (1957) as a starting point, this paper touches on the schism between developmental and social psychology and attempts to assess the progress of research in social development during the past quarter century with respect to five major perspectives that are at once evolutionary, historical, ontogenetic, cross-cultural and systemic. A pluralist view is advocated. It is suggested that a psychology of childhood must be: (1) a developmental psychology; (2) a psychology concerned with adaptation which contains an evolutionary perspective and an ecological point of view; (3) a cross-cultural psychology; (4) a psychology of social systems as well as of socializing individuals; and (5) a psychology with a historical point of view. A brief review of research in these various areas is included. (Author/MS)

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Toward a Social Psychology of Childhood: From Patterns of Child Rearing to 1984[1]

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It is now twenty-five years since data-gathering was completed for Patterns of Child Rearing (Sears, Maccoby, & Levin, 1957). The book itself was published in 1957 -- nearly twenty years ago -- marking the culmination of the sort of research on socialization that had begun to take shape within the "personality and culture" movement of the 1930s and 1940s. Several significant works in this tradition were to be published after 1957 (e.g., the Whitings' Six Cultures), but no more extensive assessment of child development in the family context has appeared since. In fact, interest in the processes of socialization has been in decline during most of the past twenty years.

No one circumstance was responsible for this decline, but several conditions contributed: a) while the personality theories of a quarter century ago were richly propaedeutic, they were weak in explanation with respect to ego (cognitive) development, b) the theories of the time did not include elaborated notions about the manner in which the child himself contributes to his own socialization, and c) the research strategies lacked predictive power. But Patterns of Child Rearing was a remarkable document. Having recently re-read large chunks of it, I remain impressed by its insightfulness, and by the elegance of the interview that Eleanor Maccoby and her colleagues created. As a documentary device, it has not been surpassed in the history of child development. But the network of antecedent-consequent relations that Robert Sears had hoped for did not emerge, nor did it emerge from Identification and Child Rearing (Sears, Rau, & Alpert, 1965) which came along nearly a decade later.

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About 1970 -- after a long hiatus -- interest in socialization research began to stir once again. Enrollments in graduate courses in social development suddenly doubled; job placements with specializations in social development went begging; and there were even claims that the socialization "half" of the Third Edition of Carmichael's Manual of Child Psychology (Mussen, 1970) should have been half of the total work instead of half as big as Volume 1. But the developmental social psychology emerging at this time is strikingly different from the developmental social psychology of twenty years ago.

Perhaps the most significant difference between current interests in social development and those of the past is the extent to which there has been a shift from socialization (i.e., the processes leading to the integration of the individual into society) to the social components of psychological development (i.e., the social components of perception and cognition, the social contextual issues in language development, the biological regulators of social activity, and the adaptational significance of social experience). Impetus for the new social development was supplied from many different quarters. Piaget's (1932) writings (particularly his early work) proved to be a goldmine. Harry and Margaret Harlow provided a ground-breaking theoretical model. Students of Lorenz and Tinbergen began to lose their trepidations (aided and abetted by the masters themselves) about applying evolutionary theory and methods to ontogenetic issues in human behavior. John Bowlby (1969) produced a remarkable synthesis in his study of attachment. And Paul Mussen (1970) provided half-a-volume at a crucial time for some needed stabs at pulling the field together.

The new breed of social/developmental psychologist is a very odd specimen: Part unreconstructed cognitive psychologist, part unreconstructed social learning theorist, and part unreconstructed evolutionist, most social development specialists are eclectics. There is much talk about "integration," but a new synthesis still lies in the future. Current studies in social cognitive psychology too frequently

resemble mere applications of basic cognitive theory to social phenomena; evolutionary advances tell us more about evolution than ontogeny; and our theories of action remain divorced from our theories about the development of social thought.

Of special concern is the lack of a liaison between developmental and social psychology. Most social psychologists acknowledge that certain individuals "out there" are interested in children's socialization, but social psychology has always been mainly concerned with attitude and influence in the social behavior of adults. As for the developmental psychologists, few darken the doors of symposia sponsored by Division 8. Everyone, of course, knows who Lewin was; but it is increasingly hard to get senior graduate students to give a coherent account of his work as it has influenced current activities in developmental psychology. And the psychology of attribution? To most, that seems to have something to do with individual differences; or, alternatively, it is a flaky kind of magical thinking in the pre-operational stage of cognitive development.

To analyze the reasons for the schism between developmental and social psychology is beyond the scope of this paper. Rather, in the remainder of this address, I want to assess where we have come in social development during the past quarter century with respect to five major "perspectives." Herein, I argue that a social psychology of childhood must be: a) a developmental psychology; b) a psychology concerned with adaptation and, as such, must contain an evolutionary perspective and an ecological point of view; c) a cross-cultural psychology; d) a psychology of social systems as well as of socializing individuals; and e) a psychology with an historical point of view.

The developmental perspective

That a social psychology of childhood should be a developmental psychology would seem to be obvious. And yet, most of the literature on children's social behavior is non-developmental. There are almost no developmental studies of social

attraction; there are very few developmental investigations of group relations; there are only one or two developmental studies of cross-pressures; and only a few age-related studies of aggression, altruism, and dependency. Most of the older theories, of course, were not developmental theories -- Lewin's wasn't, nor is social learning theory in most of its forms. Psychoanalysis provided a developmental theory of great elegance but it has been a very difficult theory to test with conventional research strategies. A few people, such as Mavis Hetherington (1967), have been able to take hypotheses derived from this theory and generate great data. But not everyone.

New impetus for developmental research in the social psychology of childhood appeared about six or seven years ago, as interest in cognitive development approached its peak. With the realization that cognitive structures might have something to do with the course of social relations (they are mentioned in Sears's famous paper, "Toward a theory of social action," published in 1951 even though most people remember that paper for other reasons), certain areas of research in social development began to change.

Among the earliest changes was research on the "origins" of social activity in infancy. Not only was attachment reassessed as a developmental phenomenon, but the fundamentals of social interaction were also explored from a developmental perspective: smiling, vocalizing, crying, following, visual preferences, auditory discrimination of social stimuli, stranger fear -- the list is very long, and it has greatly enlarged our understanding of how the social repertoire emerges. With respect to the later stages in socialization, however, we have been less consistent. In imitation research, long an area in which the principal investigators took virtually no interest in developmental parameters, the picture is now changed markedly: Zigler and his associates (Yando, Seitz, & Zigler, in preparation), have updated the survey of developmental studies that Brian Coates and I conducted in 1967, adding an excellent study of their own. The results are very interesting:

when imitation is directly invoked and involves a relatively complex task, it becomes both more frequent and more successful with increasing age. When task demands are few and spontaneous imitation is at issue, young children imitate as readily as older children. Generalized imitation thus is built-in quite early, but the imitative process is constrained in a variety of ways that only developmental research can elucidate -- by processes of selective attention, linguistic encoding, memory, perceptual-motor systems, processes determining rehearsal and retrieval of stored information, and by many others. To examine these developmental parameters within an imitation paradigm is to learn something about both information-processing and imitation, contrary to what certain critics have implied. Studies showing age differences in the effectiveness of verbal rehearsal on imitative reproduction (e.g., Coates & Hartup, 1969) are not simply studies of the development of verbal rehearsal mechanisms and of no interest to the scientist whose concerns lie with imitation.

The group relations literature is another matter. Not one of the old Lewin (1939) studies was conducted within a developmental perspective. Not one of the Sherif (1961) studies has dealt with group relations from such a perspective. None of the cooperation and competition literature is developmental and, beyond the preschool years, we have only a hazy perspective on the developmental course of group problem solving. My own work furnishes two examples of what we have been missing:

First, in contrasting the social behavior of preschool children and elementary school children in day care centers, qualitative as well as quantitative differences were found in aggressive interaction. The older children were less aggressive per unit time than the younger children but, as reported at these meetings three years ago (Hartup, 1974), the older children were relatively more aggressive in person-oriented ways (hostile aggression) than were the younger children. We were guided through this study by hypotheses stating that the cognitive complexity of hostile

insult (which is the instigation to hostile counter-attack) is greater than the complexities of the instigation to instrumental aggression. Therefore, we reasoned, hostile aggression should be more salient in the social intercourse of older children than of younger children. And it was.

Second, we have recently reported (Graziano, French, Brownell, & Hartup, 1976) that problem-solving activity in small groups undergoes developmental transformations as well. In this study, we examined both the individual and collective performance of first- and third-grade children in three-person groups. Group performance on a simple task did not differ according to whether the groups were composed of first-graders, third-graders, or mixtures of the two. But in mixed groups, individual third graders were more productive when their "peers" were first-graders, while first-graders performed similarly regardless of whether their partners were agemates or older children. Somehow, the modulation of one's contribution to group tasks undergoes a shift from the first- to the third grade and the division of labor shifts accordingly.

Further examples of the utility of a developmental perspective in building a social psychology of childhood are not needed. There is a small, but growing, literature on developmental aspects of perspective taking, on cognitive-mediational factors in social comparison, and occasional developmental studies have appeared within the past year or two on person perception, impression formation, and equity. And here is a last illustration: Bill Graziano (1976) in his dissertation completed with Ellen Berscheid, has found that first- and third-graders show an equal tendency to apportion rewards in proportion to work success, with greater reward being assigned to greater effort under most conditions. First-graders, however, suspend this equity norm when the comparison involves individuals who are both different in age and different in size. Since individuals who are different in age but not different in size are treated the same, it seems that the bigger you are the better you are! But this is a fleetingly-held norm: by the third-grade, size is

irrelevant and work is the all-persuasive determinant of equity judgments. Again, a fascinating development in children's social relations has been revealed that is not discoverable in the absence of a developmental perspective.

Comments on evolution and social development

Few investigators thought in evolutionary terms about social development in the immediate post-World War II era. Biological determinism was not a popular viewpoint. Within the ranks of the major theorists, only Freud was thoroughly deterministic. And, even here, the research based on the theory mostly had to do with the impact of the social milieu on the impulse structure rather than the reverse. By the 1960s, during the vogue enjoyed by the psychology of learning, nearly everything in social development was focused on social experience.

Now, 25 years after Patterns of Child Rearing, the evolutionary perspective is opening broad vistas. The methodology of the animal behaviorists is not, contrary to what many people believe, what developmental psychology stands to benefit from most. Good observational techniques for studying social activity in situ have been around in our field since the 1920s, and some of them still work superbly in situations into which the videocamera cannot be intruded. Rather, the ethological perspective has given us a new respect for consideration of the child in adaptational terms.

Much of the new work on the ethology of social development concerns molecular bits of social interaction: eyebrow manipulations, hand and arm movements, the expressions on children's faces (Kreutzer & Charlesworth, 1976). Such work is imperative to establish evolutionary continuities, although it is not the work that interests me most. I find the work on social organization in children's groups, patterns of social interaction, and the relation between social activity and the environment to be the most exciting material in this domain.

Years ago, Beth Gellert (Gellert, 1961) demonstrated that dominance relations in dyads composed of preschool children tended to be stable across time. Dominance hierarchies in larger groups were not studied, except as such hierarchies were implied in the results of sociometric investigations (Marshall & McCandless, 1957). Now, in the work of the Omarks (in press), the Strayers (1975), and others, a picture is beginning to emerge showing the existence of such hierarchies, albeit unstable ones, in groups of very young children. Once this work acquires an ontogenetic perspective along with an evolutionary perspective, the results should be fascinating because the functional significance of dominance hierarchies is now beginning to come clear: Rona Abramovitch (1976), in her dissertation, has found that dominance (as measured by who wins arguments) is related to the amount of attention received during non-agonistic interaction. In other words, the dominant members of the preschool play group are watched more frequently than the less dominant ones. Abramovitch argues persuasively that this association is not mediated by wariness, but is an information-gathering expedient. Here it becomes clear that only a pluralistic view of the child's social development will serve us: Abramovitch must bring a powerful information-processing analysis to bear on her problem, as well as the broad, functional outlines she has used thus far.

An evolutionary-ecological perspective also opens up vistas that have escaped us because of our laziness, our stupidity, or both. Three years ago, Vernon Allen asked me to write a background paper dealing with developmental psychology and the consequences of mixed-age peer interaction in child development. With a casual "yes" I went off to review the literature and discovered there was none! Except for some anecdotal material in Lois Murphy's studies (1937) and more anecdotal material in the Six Cultures Study (Whiting, 1963), the entire peer relations literature was a same-age literature. The explanation can only be that psychologists have been too lazy to do anything but study children's social relations in age-graded schools. A quick search of the literature revealed this to be true -- nearly every study that

was cited in my chapter in the Manual (Hartup, 1970) had been done in either an age-graded school or some other age-graded institution. And yet, Barker & Wright (1955) reported that 65% of the child-child contacts experienced by children in Midwest were between children who were not the same age. Later on, Melvin Konner (1975) reported the same thing with respect to the Bushmen.

Of course, commonly-occurring events aren't necessarily interesting just because they are common. But this situation was ludicrous. When two children differ greatly in cognitive capacity and social experience how possibly could the accomodations occurring between them resemble the accomodations occurring among agemates? At the time I was ruminating about this problem, Marilyn Shatz and her colleague, Rochel Gelman (1973), published a paper in which they reported that four-year-old children don't talk to agemates like they talk to younger children. Such accomodative capacity indicates that "peerness," even among very young children, is a relative concept: One can be peer-like even when not actually an agemate, and one can be unpeer-like even though exactly an agemate.

With Michael Lougee and Royal Grueneich (1976), I have been examining social behavior in a play situation with preschoolers that somewhat resembles one of the situations used by Shatz and Gelman (1973). We video-taped two 10-minute play sessions involving 27 pairs of preschool children who were initially strangers to one another. Three- and five-year-old same-age dyads were included in the experiment along with dyads composed of one three-year old and one five-year old. The amount of social interaction was markedly different under these conditions, with the level of interaction in the mixed-age groups standing at an intermediate point with respect to the three- and five-year old same-age groups. And the individual members of these mixed-age dyads, relative to their counterparts in the same-age conditions, accomodated "up" or "down" in terms of social activity as the case might be. The frequency of appropriate speech (Garvey & Hogan, 1973) varied in the

same way, as did the appropriateness of responding to questions. Other accomodations in linguistic structure were not so obvious. But age clearly emerged as a significant status variable: some age-related comment or other occurred in over 50% of the same-age dyads, while such comments surfaced in only 17% of the mixed-age dyads. When "you are not quite sure," you better "find out how old he is."

I am still not certain about the function of age-mixture in children's social relations. I am inclined to agree with Konner (1975) that in mixed-age conditions there is optimal opportunity for younger children to acquire information and skills necessary to their survival in a wider social world and optimal opportunity for older children to learn necessary supportive and caretaking behaviors. But I also believe that the same-age peer group serves unique functions in complex, modern cultures. I am convinced that age-grading would occur even if our schools were not age-graded and children were left alone to determine the composition of their own societies. After all, one can only learn to be a good fighter among agemates: the bigger guys will kill you, and the little ones are no challenge. Sexual experience at pubescence with bigger people is too anxiety-laden and sexual experience with littler ones is really not very interesting. And so it goes, the enrichment of our science by application of evolutionary perspectives to the social psychology of childhood.

Cross-cultural perspectives

The cross-cultural perspective in the social psychology of childhood has been in evidence for many years. The decade that saw the publication of Patterns of Child Rearing also saw the inception of the monumental Six Cultures Study which, with the publication of the Whitings' Children of Six Cultures (1975) last year, is nearing completion.

The contributions of cross-cultural methods to social/developmental psychology are the same as to psychology generally: expansion of the range of observable phenomena -- ranges of behavior, ranges of environments in which individuals live;

ranges of relations between environmental and behavioral variations. In most instances, the appeal of cross-cultural analysis is in the expanded range of variations allowed to the investigator; indeed, variations themselves oftentimes seem to be the main motivation for investigation. At other times, cross-cultural analysis focuses on universals, or the elucidation of developmental processes through experimental reduction of culture-related variations.

Cross-cultural methods and strategies are applicable to every facet of a social/developmental psychology, ranging from social cognition to the role of affect in interpersonal relations. But cross-cultural work in social/developmental psychology has been spotty, and every success has been matched with numerous failures. For example, modern methods have elucidated much about the environments in which social development occurs, including the structures of social interaction within the family, as these involve both infants and older children. The newer data support the older data in showing that the child's social behavior is embedded in a broad network of belief systems, attitudes, and ecological variations (LeVine, 1970). However, neat antecedent-consequent statements about the origins of social behavior still elude us. But such statements elude us in many areas and, in itself, this state of affairs is not particularly bothersome.

Far more bothersome, is that so many key problems have received such small attention in cross-cultural research. For example, very little is known about the role of peer relations in child development from a cross-cultural perspective. In 1970, when I reviewed this literature, I was forced to exclude the area entirely because almost nothing but anecdotal material had been published. That this gap is an important oversight is shown by the Whitings' (1975) work in which there is clear evidence that the qualities of social interaction among peers are very different from interaction involving other targets (e.g., infants or parents) and, most important, that the nature of these target differences is remarkably similar across cultures. For example, "aggressiveness," "sociable behavior," and "prosocial

activity" hold down the three most highly rank-ordered positions in peer interaction in all six cultures studied whereas "dependency," "nurturance," and "intimacy" occupied the lowest rank-ordered positions in all six. Thus, nowhere does socialization for aggression occur primarily within the family (as some of our earlier theories would have us believe) and prosocial activity, also, seems derived from peer interaction rather than from parent-child interaction. While it may be understandable that aggression occurs among peers rather than in interaction between the child and his parents (parents are both larger and vested with more authority than peers are), the fact that peers are the first- or second-ranked targets for aggression in all of these societies is most impressive. Doubly so, because sociable and aggressive behavior patterns also occur prominently in the peer interactions of the non-human primates. Beyond this, the cross-cultural literature on group relations is nearly non-existent.

Another area in which the cross-cultural literature is nearly barren is in "social cognition." Carolyn Shantz' recent review of this literature (1975) does not contain one single cross-cultural study, with the closest approximation being the sub-cultural comparisons contained in Maria Hollos' studies of Norwegian children (Hollos & Cowan, 1973). She found that children in farm families were less advanced in role taking ability than were children in Norwegian villages and cities. To be sure, Kohlberg (1969) has studied moral development cross-culturally but, otherwise, we do not know much about the interaction between cultural contingency and developmental process in determining children's social cognitive abilities.

Likewise, the literature on social attitudes, person perception, and impression formation lacks a cross-cultural perspective. Of course, there is an enormous cross-cultural literature on political and social attitudes, but it amounts to little more than a welter of findings showing cultural variations (which the investigators knew almost for certain would be there before they even looked). So, in

spite of the fact that cross-cultural perspectives have occupied a prominent place in child development research for many decades, there are enormous gaps in the application of this perspective to the problems of a social psychology of childhood.

A social systems perspective

Most investigators resist conceptualizing the social behavior of children in systemic terms. Monadic views mark the entire social cognition literature, most of the social learning literature, and nearly all of the literature in personality development. Doggedly, we continue to insist that aggressiveness, attachment, and perspective-taking are characteristics of individuals rather than components of social interaction. Just as doggedly, most researchers dealing with dominance and prosocial activity treat prediction of individual differences as the central issue.

A number of developmental psychologists have recently sounded Sears's (1951) tocsin that social behavior must be viewed in dyadic terms: Richard Bell (1969), Harriet Rheingold (1969), and others have published important papers dealing with this approach. But our social psychology of childhood is extremely weak with respect to its understanding of children as units in social systems. We have gained some understanding of children as members of families but, even there, important familial relationships (such as attachment) are not ordinarily conceived in systemic terms (Hartup & Lempers, 1973). Children's societies, those spontaneously-formed enclaves in which nearly every human child participates (Joseph Stone called them this), are understood mostly by anecdote, supplemented by an occasional bit of brilliant reporting by the Sherifs (1964). But, as noted by numerous sociologists, the variation between individuals in groups relative to the variation between groups shows the existence of system-influences in group relations that are not equivalent to the sum total of the individual roles. One discovers these systemic properties only by looking at the total activity of the members together over total time or, in other terminology, by examining the social system as well as the individuals. One

looks at transaction as well as interaction, and interaction as well as action. Thus, in our studies (Graziano, French, Brownell, & Hartup, 1976), mixed-age social systems can only be described by reference to the distribution of individual work effort within groups (which varies according to the ages of the children) in relation to the output from the groups considered as units (which does not vary according to age-mixture).

Developmental psychologists have not been very interested in pursuing the study of childhood social systems mostly because we don't know how; this conceptual skill is more traditional to sociology than to psychology. The central core of psychology has always been the individual -- or, more precisely, some attribute of individuals such as their minds, their neuronal firings, or their social actions. But I believe that this individualistic tradition has retarded integrative research in developmental social science. It is truncated to consider attachment as an affective phenomenon without considering its social systemic properties, or to consider the moral judgments of children without an analysis of the social systems from which they emerge. So, "let the sociologists do it" is not good enough. For one thing, most sociologists don't know anything about ontogenesis and, unless E. O. Wilson (1975) has a greater impact than I think he will have, most sociologists don't think too deeply about the comparative aspects of social organization.

But, if developmental psychology persists in its lofty isolation from the analysis of social structures, it will become as impoverished as it was in isolation from organismic structures. Robert Hinde, the distinguished biologist, has just published two elegant papers (1976a, 1976b) on social interactions and social structure in which he lays out a conceptual framework that builds, in a linear mode, from the basic elements (interaction) through relationships (defined by the content, qualities, and patterns existing within a succession of interactions between two individuals), to group structure (which, at both surface levels and deeper levels,

is revealed by the nature, quality, and patterning of relationships). Now that Hinde and his collaborators are studying children, there is a good chance that the systemic data that child psychologists have failed for so many years to supply will, at last, emerge. Ah well, we have been warned: The deep structures of social systems (Levi-Strauss, 1962) deserve as central a place in a social psychology of childhood as the deep structures of children's social communications.

The historical perspective

The study of child development in historical context scarcely exists. This literature, with which most of us in this room are not familiar, consists mostly of descriptions of children's environments at various points in historical time (Chambers, 1963) with emphasis on some of the more dramatic changes in Western culture as these affected children. Thus, there are important studies of child labor, child welfare, and family life in various epochs, and histories of institutions and institutional practices as these affect children -- the history of education, the history of social welfare, and the history of residential treatment. Only piecemeal accounts exist of the children themselves: their stature, their competencies, or their social behavior. Occasional studies of cohort differences among individuals are published, but not much can be retrieved in this area from existing records going back decades or centuries. Nevertheless, developmental psychology has been around long enough so that examination of certain data about social behavior within an historical perspective has become an increasingly important priority.

A case in point: Martin Richards and Yvonne Brackbill (personal communication) have recently pointed out that the data accumulated since World War II on sex differences in neonatal activity may be an artifact of historical place and time. Three considerations make the case: a) these sex difference data were collected almost exclusively in the United States, b) in this particular epoch, pediatric practice determined, at near-universal levels, that all male infants should be

circumsized within 48 hours of birth; and c) increased activity and irritability are common sequelae of circumcision. To clinch matters, new data from Britain, where circumcision is not as common, do not reveal the sex differences sometimes reported here.

Students who are interested in process elements in social development, as opposed to substantive elements, may find historical analysis to be of limited value (Sears, 1975); on the other hand, every substantive datum we possess is the product of a particular time and a particular place. Long-cited literature in our field need reevaluation, across epochs, to test its current applicability. And I stress that the test is only for current applicability because there is no such thing as the "true," "baseline," or "natural" level of behavior in the long course of human history. Unless one takes the dubious position that man has altered his environment more rapidly than evolution could keep up with, and that the "natural" social conditions for man are hunter-gatherer conditions, any one epoch is as "natural" as any other.

On very few occasions have child development data been contrasted across time: Miller, Swanson, and their colleagues (1958) attempted this with respect to the American family about 20 years ago. Students of social behavior in children rarely do so. K. E. Barnes (1972) placed a small article in the literature four years ago in which he reported a replication -- across 40 years -- of Mildren Parten's (1931-32) data on social play -- data which are cited in every American textbook on child development. Unfortunately, Barnes's study involved a sample that could never be considered comparable to Parten's and his conclusion that today's children are less sociable than the children of the late 1920s is untenable. Interestingly, however, Barnes did not comment on his most interesting findings: a replication showing the same developmental changes occurring in social play that were uncovered earlier. Three-year olds were more likely to be involved in solitary

and parallel activities than five-year olds; and the latter were more often involved in cooperative and associative interaction.

Another type of historical analysis can be attempted through which developmental hypotheses are tested by examining the relation between context and behavior across historical epochs. Antecedent-consequent relations are tested in much the same way that Whiting and Child (1953) tested them across cultures. This kind of analysis is rare, although a few people are trying it. Thus, Marcia Guttentag (personal communication) is working on the relation between the occurrence of wars and the status of women and, similarly, one could examine the oft-postulated hypothesis that educational reform and child labor legislation bear a direct relation to economic depressions. Other problems that touch more directly on the central issues of developmental psychology are: the relation between family structure and sex-role development; social climate and the origins of aggression; child care regimes and affective development. And, we desperately need to examine contemporary patterns of social change and change in parent-child relations during adolescence within an historical framework.

Concluding comment

It must now be clear that I am a pluralist. I have advocated pluralism in viewpoint, pluralism in levels of analysis, and pluralism in the use of theoretical constructs. Why am I a pluralist? Partly a matter of taste; partly a matter of expediency; but, also, partly a matter of conviction. With respect to convictions, my pluralism is founded in a firm belief that the social psychologist and the developmental psychologist should construct their fields in such a way that data will be generalizable. Knowledge, at any level (whether addressed to basic or applied problems), must be based on a broad understanding of the child's social development in context. This is not so much a matter of the formal study of the ecology of human development as it is a matter of perspectives -- perspectives that are at once evolutionary, historical, ontogenetic, cross-cultural, and systemic.

And, now, why did I titilate this audience with the apocalyptic reference in the title of this talk to 1984? Did I think that Big Brother, thought control, Newspeak, and related conditions should be the target for social psychologists who are studying developmental processes? No, I had no such intention. I simply wanted to provide a prospective dimension to the title and, in a weak moment, saw Orwell's title as one that would give a futuristic emphasis without the grandiosity of 2001. Orwell's title carries other connotations, though, and if they have fooled you, then I must apologize.

I believe in the perspective-taking urged in this paper. Signs abound that most of these perspectives will be utilized through the 1980s and onward. If so, we should have much more to contribute to society in 2001 than we have just a quarter century after Patterns of Child Rearing.

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